DISCUSSION

THE CONCEPT OF FORCE

By P. Foulkes

In his account (*A.J.P.*, May, 1951) of Hertz's motives for formulating a mechanics without the notion of force, Professor Smart maintains that Hertz was wrong in thinking that the people who used the concept of force, as opposed to those who discussed it, were confused. Having thus contrived to make the issue one of what people say about force, Smart proceeds to a linguistic discussion which not only misses the whole point of Hertz's work but also brings out the general weakness of linguistic analysis. Indeed this might well be expected since the issue concerning mechanics is not a matter of language. To understand the Hertzian point of view it is necessary to consider certain confusions in Newtonian mechanics which affect not only those who talk about this subject but also those who use it. Hertz gives some explicit examples of these confusions. Smart does not entirely overlook this, but he thinks Hertz to be in error here. Why this is so, why the practitioners of mechanical science should be immune from confusion, is never shown. Nor of course could it be shown on linguistic grounds, and here lies one of the obvious defects of linguistic analysis. For it has to rely on the false assumption that there is some current linguistic usage which is free from confusion.

This is not the place for a detailed discussion of mechanics. But to make anything of the issue we must go back at least as far as Newton's distinction between *vis insita* and *vis impressa*. It is only with this in mind that we can understand

why the laws of motion in the *Principia* came to be stated in their peculiar form. The *vis insita* or inertia of a body is its property of resisting motion or change of motion. The definition suggests that this force is inherent in the body. Corresponding to it we have the first law of motion which states in effect that under *vis insita* alone motion, if it occurs at all, is uniform and rectilinear. *Vis impressa* on the other hand is what interferes with uniform motion and this appears in a sense as a characteristic activity of some other body. In the second law of motion we have then the statement that under *vis impressa* motion is accelerated in the direction of impression in inverse proportion to the mass of the moving body. As an afterthought, we are then given the law of action and reaction, which must be stated explicitly, since force was considered as being something qualitative.

If we recognise the relational character of force this becomes superfluous, for interaction is a symmetrical relation. This point is brought out in Hertz's treatment of force (pp. 184-185 of his *Principles of Mechanics*) as also is the sort of confusion caused by the Newtonian formulation (pp. 5-6). This kind of difficulty could not possibly arise in a straightout relational account of force. On such a basis it is clear that the number of laws of motion will be reduced to one. For the above-mentioned Newtonian distinction disappears and we are simply left with the occurrence or absence of interaction, and one law specifying these. In Hertz this single proposition is formulated so as to refer to the motion of a closed system, or free system as he calls it, in a statement somewhat analogous to Newton's first law. He then considers free systems as consisting of two coupled partial systems each of which is of course unfree, or open, and force is defined as "The independently conceived effect which one of two coupled systems, as a consequence of the fundamental law, exerts upon the motion of the other" (§ 455, p. 185). From this it follows at once that to every force there is a counterforce, and their numerical equality and opposition in direction are easily estab-

lished. Furthermore, the relational treatment obviates the distinction between static and dynamic forces and the artificial connection which on the old theory has to be made between them. The Newtonian theory here needs over and above the laws of motion the introduction of d'Alembert's principle. The Hertzian treatment requires no additional principle, it gives a single account of the two types of interaction.

Without going into further detail it is clear now why the Newtonian formulation, however modified, leads to further difficulties, as shown for example in Smart's discussion of the first and second law. As far as the Newtonian theory is concerned they are empirical laws and not a definition of force. But the Newtonian concept of force is confused, it presents force as a quality instead of recognising it as a relation. That is the real source of the confusion, and however we twist the matter, the account raises difficulties. For Hertz such difficulties do not arise because his relational account removes the source of confusion. His fundamental law (§ 309, p. 144) is of course synthetic. The question "analytic or synthetic?" is thus answered: the laws of motion are synthetic. The question is misleading only in so far as it suggests that there are analytic laws at all.

If we treat force as a relation it becomes clear also why we cannot define it in the ordinary way in which we define qualities. It is necessary here to distinguish between Smart's linguistic notion of definition and the traditional notion of definition by genus and difference. For Smart definition is simply the substitution of several words for a single "shorthand" word, and he points out quite correctly that most words are not shorthand words. Definition in this sense is in fact not very important. But whereas verbal substitution can only be applied to shorthand words definition by genus and difference can be applied without restriction to any qualitative term. It is this sort of informative definition that Hertz had in mind when he considered the question "what is force?" And although he did not see it clearly, his own treatment of force

implicitly points to the answer. Force is not a qualitative term but a relation between such terms and therefore no definition can be given of it.

In terms of genus and difference, every qualitative term has a variety of definitions, none of which will tell us everything that could be said about the definiendum. There is then no dichotomy of definable-indefinable in this sense of definition. But, quite apart from this, Smart's contrast between the circle, definable in the linguistic sense, and the electron, allegedly not so definable, cannot be maintained. In mathematics, he says, the dichotomy definable-indefinable works. There are certain indefinables implicitly defined by the axioms. For the rest only shorthand concepts occur. "Circle", for instance, is shorthand for "curve everywhere equally distant from a given point". But instead of attempting to show why this dichotomy fails elsewhere, Smart simply says that where language has an empirical job to do its form is such as to make the dichotomy "ludicrously inappropriate", and he goes on to suggest that "electron" could not be defined. A definition in his sense would be a set of hard and fast rules for the use of "electron". This being regarded as impossible, we can at best give more or less strict explanations of the term and in due course we may acquire linguistic competence in its use. But the whole illustration merely serves to underline the weakness of the linguistic approach. For in the first place "electron" is definable in the linguistic sense, for instance as "the carrier of the least electric charge". This form of words could be substituted wherever the word "electron" occurs in a textbook on physics. In the second place we see from this example that the verbal definition here is not just verbal, it is informative as well, although it does not tell us very much. From Smart's recognition of the fact that very few words are shorthand it is in any case clear that in the end a verbal definition involves terms that make it an ordinary definition in the traditional sense as well. But further, once we have given the above definition of "electron", there is absolutely no difference

between the case of the circle and that of the electron. The definition in either case tells us something about circles and something about electrons. For the rest it is the business of mathematical science to discover the properties of circles just as it is the business of physical science to discover the properties of electrons. The process of discovery is the same in both fields. The so-called indefinables of mathematics are merely undefined, in the same way as the so-called axioms are not unprovable but merely unproved, which does not mean that there are unprovable propositions any more than indefinable terms. An unprovable proposition would equally be undisprovable and thus it could not enter relations of implication at all. An indefinable term would equally be inexcludable and thus it could not function in a proposition at all. It is true of course that we cannot prove and define everything at once; something is always taken for granted without proof or definition. Considerations of this type followed up in detail dispose of analytic propositions and indefinable terms, and, as a consequence, of non-empirical accounts of mathematics.

In any case the whole question of linguistic usage and competence is beside the point. It is an incidental question and is settled incidentally as enquiry proceeds. Moreover it is to be remembered that the question of how certain linguistic usages gain prominence is not itself a linguistic question. Without attending to this latter non-linguistic question it is impossible to see the point of even a linguistic confusion. This, it seems to me, is in some measure apparent in Smart's article. Quite generally, however, the weakness of linguistic casuistry stands out when we consider the process of acquiring linguistic competence. For to learn to speak "correctly" is not just a matter of learning to communicate intelligibly. It involves also the taking over of established traditions and along with them their attendant prejudices and confusions.

To remove linguistic confusions it is necessary to examine what is being asserted or claimed when they occur. We then find either that certain propositions supposed true are in fact

false, or that their terms presuppose the truth of propositions which are in fact false, or finally that a term is taken qualitatively when it is relational or *vice versa,* and along with it goes the ambiguous use of a term as being both. It is this last sort of confusion—Anderson calls it the relativist confusion—that is the most serious. We have seen it operating in mechanics. In logic itself it gives rise to the notion of analytic propositions through the confusion of relations of necessity between propositions with an alleged character of necessity.

These in brief are some of the issues arising in connection with any enquiry. To appeal simply and solely to linguistic rules does not even touch the problem. The discussion on Hertz's mechanics illustrates this point. Confusions in language do indeed occur. In order to remove them, however, we need to do more than refer to usage, we have to go behind it and examine what is taken for granted and what is being asserted or denied. For usage is confused itself, and an appeal to it just makes confusion worse confounded.